

Interactive comment on “Response of hydrology and CO₂ flux to experimentally-altered rainfall frequency in a temperate poor fen, southern Ontario, Canada” by Danielle D. Radu and Tim P. Duval

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We thank both reviewers for their constructive comments on our submitted manuscript. We have considered each point carefully and made the suggested changes or provided greater detail into our justification for not wholly adopting an idea. Overall, we feel the revised manuscript is more structurally sound and informative for the international biogeoscience community. We have reduced the number of figures by two and removed a table from the main manuscript and moved them to the supplemental material. We have reworked and removed most of the treatment of the field study, as suggested

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primarily by Reviewer #2. We have added discussion to the underlying mechanisms driving our results in a number of instances based on comments of Reviewers #1 and #2. Finally, we are in the midst of rerunning our statistical analysis to ensure the correct approach is utilized.

The detailed responses to the reviewers' comments are below, with their comments in quotations and our responses on a separate line preceded by three asterisks: ***. We have also uploaded our working revised manuscript with highlighted changes.

We thank the associate editor, Paul Stoy, for his efforts with handling this manuscript and welcome further feedback.

Thank you,

Tim Duval (on behalf of my co-author, Danielle Radu).

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"Alongside significant results, the submitted article also provides some important baseline information with concise descriptions which is useful for climate model simulations. The objectives are clear and analyses are also straightforward and easy to comprehend. At the same time, however, I do feel that some parts are missing while reading through the manuscript."

"I see the authors describing field experiment which includes the CO<sub>2</sub> fluxes measurement (line 105-113) but not discussing the results of this experiment. So, I expect further descriptions about this in the manuscript and also objectives of conducting two experiments."

\*\*\*Based on this comment, and some of the comments of Reviewer #2 we have decided to take a different approach than the one suggested by Reviewer #1. We have decided to remove the methodology of the field study, as well as the presentation of the relevant results of the field study in the present manuscript (Figure 6, Results L234-239). The field study is an article currently in press; in the article we present seasonal changes

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to vegetation and model CO<sub>2</sub> balances as a consequence of altered rainfall pattern. The field data presented in this study were analysed the same way as the lab study, and was meant to support the controlled-environment of the lab study. There was no separate objective to the field study.

"The authors mentioned the lowering of WT increases respiration and switching the communities to net CO<sub>2</sub> source or C neutral. I think it would be better to elaborate a bit on the mechanisms driving the increased respiration."

\*\*\*We have added text to the discussion (L323-326 and L329-331) to highlight the mechanisms of increased respiration with lowered WT due to greater aeration.

"I will point out some other minor comments which I feel could be improved."

"Line 230: The magnitude of NEE under low WT was greater during frequent rain than during the MedFreq and LowFreq. It would be good to describe the meaning of 'greater NEE' here."

\*\*\*We have added a clause to this sentence to highlight that the greater magnitude of NEE for the HiFreq treatment refers to more net carbon uptake (new Line 243).

"Line 261: It doesn't look like -29 cm in Fig. 8c."

\*\*\*The lowest NEE rate (least negative, -29 cm) stated for the HiFreq treatment in Fig. 8c is the computed value of the quadratic regression model. We agree that the data cloud doesn't necessarily agree with this computed value. We also note that while significant ( $p < 0.001$ ) the correlation was rather weak ( $R^2 = 0.15$ ). Thus, we have modified this statement to depict the more general trend of the data.

"Figure 3c: The negative sign of the slope for high WT is missing."

\*\*\*Thank you for noticing this omission. The negative sign will be added to our final submission.

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Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2017-485/bg-2017-485-AC1-supplement.pdf>

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Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-485>, 2017.

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